this plane; and let the motion be referred to S X, S Z, and a line perpendicular to them as axes. Now in consequence of the proper motions of the sun and the two stars, these lines will not constitute a system of axes which are either fixed in space or which move parallel to themselves; consequently, if these lines be taken as axes of reference the observed (or calculated) value of the motion of rotation of any object will necessarily be *relative* to these axes, and its *absolute* value cannot be determined without ascertaining the angular motion of the axes of reference. But since the two stars have been specially selected from amongst those whose proper motions have eluded detection, the angular motions of the axes will be so small compared with ordinary standards that the error caused by neglecting them will lie within the limits of experimental error. If, for example, we were to endeavour to calculate by the above method, or some similar one, the *absolute* angular velocities of the hands of a clock, or of a fly-wheel, the value which we should thereby obtain would be as exact as the existing state of mechanical and instrumental appliances admits.

A series of observations extending over several thousand years might of course reveal a proper motion of any two selected stars; but in the case of angular motions whose periods do not extend much beyond a century, the above method leads to results which are for most practical purposes exact.

I am therefore disposed to think that all motions of rotation of which our senses are capable of taking cognisance are *relative;* but with the exception of certain astronomical motions of very long periods, the *absolute* value of any motion of rotation which can be observed may be accurately determined within the limits of experimental error. A. B. BASSET.

Fledborough Hall, Holyport, Berks.

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THE gist of Mr. Love's long letter, on pp. 198–9, seems to be that since the specification of a force or a motion depends upon the choice of axes, therefore the force or motion itself is similarly dependent. A slight extension of the same principle would make a velocity—say the velocity of light—depend upon whether it was to be expressed in miles per second or in centimetres per hour; and no extension at all is necessary to make it depend on whether it is referred to Groombridge 1830, or to some more quiescent body, if indeed the term quiescence may be allowed henceforth to have any meaning.

Tycho Brahé is said to have held that there was only a question of language between the Copernican and the Ptolemaic systems; but, with the exception of a semi-ironical Church compromise attributed to Descartes, he has been unfortunate in not finding a disciple of importance until the present moment.

It appears now to be equally true to say that the earth rises to meet a stone, as to say that a stone falls to meet the earth; and considerations of energy are of no consequence!

I just want to add one word of my own on the subject, to the effect that whereas the *position* of a body in an infinite homogeneous stagnant ocean would be unmeaning and unspecifiable, except by reference to boundaries or other bodies, yet it does not follow that the *velocity* of a body through such an ocean would be either unmeaning, unspecifiable, or undiscoverable by experiment. It may be replied that such motion would still be relative to something; and to that I say by all means, but it is not relative to other bodies such as are competent to fix *position*, which is what Mr. Love is contending for.

As to the other question, about absoluteness of rotation, I shall be much interested in seeing what Prof. Greenhill, and with him Profs. Mach and Karl Pearson, have to say on the point. OLIVER J. LODGE.

Liverpool, December 29, 1894.

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### The Quarrying of Granite in India.

At Bangalore, in Southern India, the quarrying of granite slabs by means of wood fire has been brought to such perfection, that an account of the method may not be out of place. The rock is a grey gneissose granite of very irregular composition through unequal segregation of hornblende and the presence of numerous felspathic veins. But it is otherwise very compact, and forms solid masses uninterrupted by cracks for several hundreds of feet. Only near the surface the rock is found split parallel to the surface. In one quarry there is thus, for instance, a 4-feet thick horizontal layer of rather weathered rock, underneath this another layer of fresh rock 3 feet thick; but below this the rock is entirely fresh, and not split. These layers are probably due to the variations of temperature, daily and seasonal.

The undisturbed rock is quarried by means of fire, and it is remarkable what large plates may be detached. I saw one plate of 60 feet greatest length, and 40 feet greatest width, and half a foot thickness. This thickness varied only one inch over the greater part of the area. The whole plate had been detached in one piece by means of wood-fire. Afterwards the plate was cut with blunt chisels into strips of 2½ feet width. So easily are these strips and slabs obtained, that it is quite common to see palisades of them used instead of boundary walls, and also to see them used as posts for huts, for telegraphs, and for railings and posts in gardens.

In one case, I observed the operation of burning over an area. A narrow line of wood-fire, perhaps 7 feet long, was gradually elongated, and at the same time moved forward over the tolerably even surface of solid rock. The line of fire was produced by dry logs of light wood, which were left burning in their position until strokes with a hammer indicated that the rock in front of the fire had become detached from the main mass underneath. The burning wood was then pushed forward a few inches, and left until the hammer again indicated that the slit had extended. Thus the fire was moved on, and at the same time the length of the line of fire was increased and made to be convex on the side of the fresh rock. The maximum length of the arc amounted to about 25 feet. It was only on this advancing line of fire that any heating took place, the portion which had been traversed being left to itself. This latter portion was covered with the ashes left by the wood, and with thin splinters which had been burst off. These splinters were only of about ⅛th inch thickness, and a few inches across. They were quite independent of the general splitting of the rock, which was all the time going on at a depth of about five inches from the surface. The burning lasted eight hours, and the line of fire advanced at the average rate of nearly 6 feet an hour. The area actually passed over by the line of fire was 460 square feet, but as the crack extended about 3 feet on either side beyond the fire, the area of the entire slab which was set free measured about 740 square feet. All this was done with, may be, about 15 cwt. of wood. Taking the average thickness of the stone at 5 inches, and its specific gravity as 2·62, the result is 30 lbs. of stone quarried with 1 lb. of wood.

The old quarries have sloping sides formed of steps left by each successively split plate, each new plate extending to within about 2 feet of the step left by the preceding plate. Many plates are taken out in an inclined position, and as the directions of inclination differ, it follows that the action of the fire is quite independent of the original surface of the rock, and also of the direction of lamination and of the numerous veins in the rock. The action of the fire is thus very similar to that action which produces dykes and faults on a large scale, more or less independent of the nature of the rocks which are passed through.

The great uniformity of the thickness of the slabs formed by the above process is probably due to a regulating influence of the pre-existing crack. When the action of the fire is somewhat slower, it takes longer for the heat to penetrate down to the crack; when the action is quicker, there will be enough expansion produced in the upper layers, and the lower layers transmit the tension to the plane of the crack. Perhaps it will be possible some day to measure the temperature of the heated rock, when a certain agreement ought to be found between the tensile strength of the rock and the strain which the expansion by the heat produces in the so-far elastic rock.

Bangalore, December 19, 1894. H. WARTH.

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### Storm Statistics at Bidston.

THE Liverpool Observatory, erected at Bidston, on the Cheshire side of the estuary of the Mersey, stands on a slight eminence about 200 feet above the sea-level. The ascent is tolerably steep on each side except from the south, and with the Irish Sea on the north, and the rivers Mersey and Dee on the east and west sides respectively, there is nothing to obstruct or diminish the force of a passing storm. Self-recording anemometers of the Robinson and Osler types have been in position since 1867, and it is from the records of these instruments that

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these statistics have been prepared. How far these records are absolutely correct is beside the present question; all the data have been obtained with the same instruments, and are strictly comparable between themselves.

It has been the custom at the observatory to treat as storms or gales, all occasions in which the Robinson anemometer recorded the horizontal velocity of the atmosphere as equal to or exceeding fifty miles an hour. The total number of instances in the twenty-eight years under consideration is 321, or an average slightly below one a month. It will frequently happen in stormy periods that the critical velocity will be registered, and be followed by a partial lull in the storm, to be succeeded by gusts of greater force. If these periods of comparative calm have lasted for about twenty-four hours, the disturbance would count as two storms, although probably both are parts of the same atmospheric disturbance.

The greatest number recorded in a year is in 1868, when no less than twenty-eight gales were reported; and the least occurs in 1880, when there were only two, and these neither long in continuance nor great in violence. It is not without significance—though, of course, it is not intended to insist upon the coincidence—that when the number of storms is plotted as an ordinate with the time for abscissa, a rough curve can be drawn among them giving maxima at practically equal intervals of five years, from 1868, with minima at intermediate dates. Or, if we take the sums of the maximum years 1868, 1873, 1878, &c., it is found that in the six we have records of eighty three storms; while in the six years of minimum, 1871, 1875, 1880, &c., we have only thirty-seven.

It is scarcely necessary to refer to the direction in which storms approach the observatory, after what has been said of its geographical position. Roughly, they are all from the west, with slight deflections to the north and south. As a matter of fact, only five have deviated from this rule, and they have been either east or south-east, and have been comparatively slight in their character.

The time scale on which the velocities have been registered is not a very open one. The recording drum moves through rather less than one inch in an hour, and the habit has been to read and record the distance travelled from the commencement of one hour to the beginning of the next. The maximum hourly velocity is, therefore, not to be understood as the greatest in any sixty consecutive minutes, but as the greatest in one whole hour as marked by the clock. In this sense the following table, which exhibits the number of times the greatest hourly velocity has exceeded noticeable amounts, is to be understood:—

| Recorded hourly velocity in miles. | No. of instances. |
|---|---|
| 50–60 | 220 |
| 60–70 | 68 |
| 70–80 | 21 |
| 80–90 | 10 |
| Exceeding 90 | 2 |

Here again, curiously enough, we find some slight evidence in favour of a five-year period. Not only do the two instances of the greatest velocity recorded in the observatory occur in years already noticed as those of maximum disturbance, but the average velocity of all storms in the years 1868, 1873, &c., is 59·2 miles, as compared with 57·9 miles in the years of minimum number.

Intimately connected with velocity, though probably a less accurate measure of the true force of a storm, is the pressure recorded per square foot. Here Bidston has long held a record for the British Isles, having placed to its credit a pressure of ninety pounds on March 9, 1871. The accuracy of this measure has often been questioned, and probably it is too great owing to the momentum in the moving parts of the machinery, but it is certainly the record of a far greater pressure than has ever been witnessed since. Considering over what small areas these excessive pressures are exercised, and the great variation that exists from moment to moment in the velocity of the wind when a storm is raging, it is not an impossible amount, but it would certainly be misleading to conclude that such a pressure was a measure of the force of the wind a few feet, or even a few inches, away from the pressure-plate. The hourly velocity on the occasion when this pressure was registered has frequently been exceeded, without reproducing similar pressures. Herein is represented the great difficulty in determining a simple relation between pressure and velocity, or, rather, the square of the velocity; for one may regard Hutton's law of wind-pressure on a given obstructing surface as satisfactorily proved. The fact of such accidents as that referred to destroys any value that can be drawn from averages, but as a mere matter of figures, it appears that the average maximum hourly velocity for all storms is 58·4 miles, and the mean of the maximum pressures 37·6 lbs. This would require the factor for multiplying the square of the velocity in miles, to obtain the pressure per square foot, to be greater than one-tenth, which is evidently and necessarily erroneous, since we are comparing the accidental momentary pressure with the average velocity obtained throughout the entire hour. Taking the extreme pressures for what they are worth, the numbers come out as follows:—

| Pressure in lbs. on sq. ft. | Number of instances. |
|---|---|
| 20–30 | 132 |
| 30–40 | 111 |
| 40–50 | 50 |
| 50–60 | 11 |
| 60–70 | 12 |

On two occasions the pressure registered was greater than 70 lbs. to the square foot, and on three did not reach twenty. Since the extreme pressures are in a sense accidental, and do not represent with any accuracy the force of a storm, it does not seem desirable to determine the relative pressures in what have been called years of maximum and minimum storm occurrence. But the general features are again borne out. The explanation here is probably that the greater the number of storms the greater the chance of finding a high pressure. It seems more profitable to inquire what is the average length of a storm, how long may a violent disturbance be expected to last. This question is unfortunately complicated by the fact already alluded to, that a storm may subside for a few hours and then reappear with its original violence. If the interruption is only for an hour or two, as already explained, the depression is considered as one, but only those hours are counted in which the registered velocity exceeds fifty miles. There are only two instances in which this amount of violence has been maintained for thirty consecutive hours, viz. in February, 1868, and again last February. The total number of stormy hours in the twenty-eight years is 1732, giving an average of 5·4 hours for each storm. Our local disturbances are therefore not of long duration.

An examination of the dates when gusty weather is most prevalent, goes neither to substantiate the ancient myth of the equinoctial gales, nor to uphold the evil supremacy which has long been assigned to the winds of March. Since no attempt has been made to equalise the lengths of the months, February has been somewhat unfairly treated in the following table; but notwithstanding this handicap, it possesses the unenviable privilege of compressing within its shortened limits more tempestuous weather than any other month.

| Month. | No. of storms. | No. of stormy hours. |
|---|---|---|
| January | 47 | 260 |
| February | 42 | 281 |
| March | 47 | 238 |
| April | 14 | 63 |
| May | 7 | 27 |
| June | 3 | 19 |
| July | 7 | 21 |
| August | 17 | 65 |
| September | 16 | 77 |
| October | 26 | 180 |
| November | 44 | 254 |
| December | 51 | 247 |
| Total | 321 | 1732 |

Of course one would expect to find the greatest number of storms in the winter; but that three-fourths of the whole should be compressed within five months of the year, is a greater disproportion than was expected. The variations of the barometer during these storms, and the dependence of this variation upon the direction of the wind, are of considerable interest, but may not very well be entered upon here while treating simply of numerical statistics.

William E. Plummer.

## Peculiarities of Psychical Research.

Mr. Dixon asks in his first letter: Could an abnormal distribution of the cards affect the result, if certain precautions were taken? In his second letter he says there was nothing in his first letter to indicate that he under-estimated the import-

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